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September 11, 1956

Dr. Joshua Lederberg
Department of Genetics
University of Wisconsin
Madison, Wisconsin

Dear Josh:

Thank you very much for reading my manuscript, and for your sympathetic comments. I saw Calvin's article in the American Scientist; not only is it "along this route", it is my central idea, so my original hopes of winning renown with the idea are dashed. I have been working on it since December 1953, and until I found who Calvin was, I suspected he had unconsciously or indirectly absorbed the idea from a ~~manuscript~~ letter I sent to Horowitz in January, 1954. *you saw* *from manuscript I sent him in March, 1954*

The manuscript (polished and revised) was sent to Dunn in June and returned yesterday with a lot of very annoying, trivial criticisms that never went near the important parts of the article. I have tried to clarify the points the reviewer misinterpreted, and to explain ^{for example} why the spontaneous synthesis and self-replication of nucleoprotein genes in an inorganic world is still a problem of sufficient magnitude to be bypassed in our theories and research, if possible. *I am still pessimistic about its publication in The Naturalist.*

I agree that it is time to do experiments on this. Experiments to date have been seeking quick results--~~in~~ mainly in synthesis of the initial building blocks. What we need to do ~~is~~ now is see what will happen to the building blocks, ~~when~~ given a chance to interact in a large variety of favorable circumstances. This is obviously going to take more time and trouble, and I am not in a position to undertake such a ~~padding~~ project by myself. But I felt if I published the idea together with suggestions for experimental testing, someone might be interested in trying it. Also, I think the idea has important implications for genetic theory, quite apart from the origin of life. For this reason, I felt my mathematical computations (which took a terrible lot of time) constituted sufficient "experimental justification".

I was pleased to meet Morton in Storrs last month. No matter what he does hereafter, his name has, I think, been permanently established in the field of human genetics by his brilliant papers on linkage.

Thanks for the extract from the Amherst article. If you hear of anybody willing to undertake experiments on "reflexive catalysis", please let me know. Give my regards to Ester.

Best wishes,

Gordon Allen.

E. The reason I never corresponded with you before about this is that I didn't suppose your interest or experience would be closer to it than Bernice's or Warner's, with whom I had always been in close touch.